OUR BOOK SHELF.

The Natural History of Plants, from the German of Prof. Anton Kerner von Marilaun. By Prof. F. W. Oliver, M.A., D.Sc. (London, Glasgow, and Dublin: Blackie and Son, 1894.)

THE high praise we gave to Prof Kerner's *Pflanzenleben* when it appeared, makes it almost unnecessary for us to say much about the English edition now in course of publication, and which will be completed in sixteen monthly parts. The German work was said in these columns to be "the best account of the vegetable kingdom for general readers which has yet been produced." This judgment can also be applied to the translation which Prof. Oliver has made, with the assistance of Miss Marian Busk and Miss Mary Ewart. In translating a work, some of the brilliancy of the original is necessarily lost. It is difficult, however, to find awkward expressions in the pages before us; in fact, very few of the idiomatic phrases of the original work have survived. And the translation is scientifically accurate, as well as entertaining and instructive. Lovers of nature will find every page of the book interesting, and the serious student of botany will derive great advantage from its perusal. The illustrations are beautiful, and, what is more necessary, true to nature. The complete work contains about one thousand engravings on wood, and sixteen plates in colours. Botanical science will benefit by the issue of Prof. Oliver's edition of a splendid book.

Notes on some of the more Common Diseases in Queensland in relation to Atmospheric Conditions, 1887–91. By David Hardie, M.D., Hon. Physician Hospital for Sick Children, Lady Bowen Maternity Hospital, Brisbane. (Brisbane: Beal, 1893.)

THE author of this work has a most important aim in view, viz. to establish the connection between the weather and the prevalent diseases in Queensland, and expresses a hope that, in time, he will be able, if furnished with a forecast of the weather, to predict with certainty the diseases likely to predominate during the various seasons of the year, and thus to lay the foundations of a practical system of preventive medicine.

The conclusions are so interesting that some of his leading results may be briefly given.

The annual death-rate of Queensland per 1000 population is 15·11, varying from 13·38 in August and September (spring) to 16·28 in November to March (summer and early autumn), as contrasted with the death-rate of Great Britain for the year, 18·8.

The yearly mortality is lowest in West Southern Queensland (Darling Downs and Warrego), where it is only 8·92 per 1000, a little over half of the average Queensland rate, and the highest is 34·70 in West Northern Queensland (Normanton), a tropical region at the extreme north of the colony. This would point to the great advantage of altitude, combined with dryness, as seen on the Darling Downs, over marine influence and moisture to be found in the sea-coast districts. When we come to different classes of disease, we find that diphtheria, though specially prevalent in April, May, June, and July (winter), is endemic to some extent in all seasons, and causes a mortality of 2·15 per cent. Whatever contributes towards cold and dampness of the air during the autumn and winter, causes an increase in the death-rate from diphtheria, and, according to Dr. Hardie, the neighbourhood of swamps and marshes have considerable influence in this respect. Whooping-cough, on the other hand, attains its maximum during the warm and moist months of the year, and its close connection with a medium temperature for all seasons of the year with high relative humidity, is considered to support the assumption of its germ origin.

Phthisis is common along the eastern portion of the colony from Cooktown to Brisbane, reaching a maximum of 12·86 per cent. in the Rockhampton district; but this high mortality is partly to be attributed to the large Polynesian population, employed on sugar plantations in these districts, who are specially liable to phthisis. In the western and northern districts, however, it is much less prevalent, and the average percentage of deaths from all causes gives to phthisis 8·75. The months when the mortality is greatest are July, August, and September, and there seems to be no special connection between atmospheric moisture and phthisis mortality, but a low temperature in summer and autumn is favourable to a low phthisis mortality. Acute respiratory diseases, such as pneumonia, pleurisy, and bronchitis, are observed all over the colony, and vary in mortality in different parts; the highest on the coast and the lowest inland, the months of highest mortality being June, July, August, and September; the maximum is reached during and immediately after the colder period of the year.

The book, with its copious and valuable tables, is an honest attempt to deal with a very difficult problem, and thoroughly merits success, and if the author will only persevere in his researches, still more important results may follow.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Panmixia.

I AM much obliged to Prof. Weldon for having so promptly answered my request, and hope that his example will be followed by any naturalists who may have any other grounds for questioning the doctrine of Panmixia. Meanwhile, however, there are two or three points touching which I should like to be sure that we correctly understand each other.

(1) Hitherto all naturalists who have written upon the subject have agreed, that "*the survival-mean must* (*on cessation of selection*) *fall to the birth-mean.*" And, in now questioning this view, Prof. Weldon appears to contemplate the difference between birth- and survival-means of only the first generation, which would be very unfair. Again, I do not follow Prof Weldon's meaning in what he says with regard to another point. Assuredly "every statistician knows" that selection can maintain the "mean height of a regiment" at 67½ inches, by enrolling only those individuals who are either "more than 66 and less than 69 inches high." But this would be *artificial* (*i.e.* intentional) selection. The "cases" to which he alludes, where *natural* selection could destroy individuals nearest the mean line, while favouring those which lie at greater distances *both above and below* this line, must be very exceptional.

(2) As regards the second cause of degeneration under Panmixia (viz. atavism), Prof. Weldon says merely that it is "not demonstrated by any statistics." This is true enough. But the same has to be said of natural selection. Whether in the building up of a structure by natural selection, or in the subsequent breaking down of a structure by atavism on the withdrawal of selection, the statistical method is equally unavailable for testing either theory: in both cases the most effective variations (*i.e.* deviations from the mean) at any given time are those which are most *numerous*, and therefore most *minute*. Hence, in both cases the best "demonstration" of the theory which can be offered is that which is yielded by the parallel facts in our domesticated animals.

(3) The only objection which is urged by Prof. Weldon against the last of the three causes which I mentioned (*i.e.* irregularities of heredity when uncontrolled by selection) is one which tells against the theory of Panmixia only because it does so against that of Natural Selection. As I understand, the argument is, "Natural selection is in most cases an imperfect agent in the adjustment of organisms": *ergo*, the cessation of

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selection will not, in most cases, make much difference in the maintenance of such adjustment. Obviously this ground of objection to the theory of the cessation of selection opens up a much larger question than can here be dealt with, viz. the adjusting or eliminating value of the presence of selection. But if Prof. Weldon will read what I wrote last year in the *Contemporary Review*, during the Spencer-Weismann controversy, he will find that in this matter I am quite on the side of Mr. Bateson and himself. It has always been my endeavour to argue that the ultra-Darwinian school of Wallace and Weismann are pushing deductive speculation much too far in maintaining "The All-Sufficiency of Natural Selection." I shall never believe—any more than Darwin believed—that what I have called "selection value" is unlimited. But this is not incompatible with the belief that *in whatever degree* natural selection may have been instrumental in the construction of an adjustment, *in some degree* must its subsequent cessation tend to the degeneration of this adjustment, especially where complicity as distinguished from size is concerned, as stated in my last letter.

Summing up his objections to the doctrine of Panmixia, Prof. Weldon says they are two: "First, it is based on the assumption that selection, when acting on a species, must of necessity change the mean character of the species—an assumption incompatible with the maintenance of a species in a constant condition." This refers to the paragraph of his letter which, as already stated, I do not understand. The doctrine of Panmixia, as far as we are now concerned with it, does not refer to "species," but to specific characters, *i.e.* structures, organs, instincts, &c. Again, the doctrine, even with regard to specific characters, makes no "assumption" touching the *presence* of "selection acting on a species"—least of all that such *presence* will not maintain the species in a constant condition. On the contrary, the very essence of the doctrine is, that it is the *presence* of selection which *maintains* the constancy of a species (or specific character), and *therefore* that it is the *cessation* of selection which *upsets* the constancy by withdrawal of the maintaining influence. Hence, I do not understand Prof. Weldon's first objection. His second is, "that in the only case which has been experimentally investigated, the condition said to result from a condition of Panmixia does not, in fact, occur." This one case, he explains, is:—"Mr. Galton has shown that civilised Englishmen are themselves in a condition of Panmixia, at least with respect to several characters, especially stature and the colour of the eyes. Now the mean stature of Englishmen is known to be increasing, and there is no evidence of the disappearance of coloured eyes." But, as regards stature, it can scarcely be maintained that there is not *some* cause at work to account for the increase; yet, unless this is maintained, the case is clearly irrelevant. Again, the colour of the eyes of our mixed population cannot have had more than thirty or forty generations wherein to be affected by Panmixia, and therefore the most ardent supporters of this doctrine would scarcely expect any result to be yet appreciable in the case of so pronounced a racial character. Surely a better "case" is the one which I have already given in the most ancient and the most rapidly-breeding of our domesticated animals. It was the facts observed in this "case" which first suggested to me the doctrine of Panmixia, and so led me to question the inherited effects of disuse. Similarly, a year later, Mr. Galton, in his "Theory of Heredity" (which anticipated by about ten years all the fundamental parts of Weismann's), wrote of Panmixia thus:—"A special cause may be assigned for the effects of disuse in causing hereditary atrophy of the disused parts. It has already been shown that all exceptionally developed organs tend to deteriorate; consequently those that are not protected by heredity will dwindle. The level of muscular efficiency in the wing of a strongly-flying bird is like the level of water in the leaky vessel of a Danaid, only secured to the race by constant effort, so to speak; let the effort be relaxed ever so little, and the level immediately falls. . . . That this is a universal tendency among races in a state of nature, is proved by the fact that existing races are only kept at their present level by the severe action of selection." George J. Romanes.

Oxford, May 5.

P.S.—I gladly accept the verbal correction in Prof. Weldon's third paragraph.

Physiological Psychology and Psychophysics.

Owing to my bookseller's habit of forwarding Nature in monthly batches, I have only just seen the remarks appended to my letter in the issue of March 15. I think that the terminological question is sufficiently important to warrant a reply to these.

(1) I do not, of course, "subsume" psychophysics to physiological psychology. The latter, I stated, is both wider and narrower than experimental psychology; and wider, because it includes the consideration of certain ("the most important") psychological problems—not of *all* such problems. (For this view of physiological psychology, *cf.* Wundt, "Physiological Psychology," fourth edition, I. p. 9.)

(2) Fechner, "the coiner of the word," defines psychophysics as "eine exacte Lehre von den functionellen oder Abhangigkeitsbeziehungen zwischen Körper und Seele, allgemeiner zwischen Körperlichen und geistigen, physischer und psychischer Welt." (*cf.* "Psychophysik," second edition, I. p. 8.) What my critic says on this head is, therefore, incorrect.

(3) In the most widespread and important school of experimental psychology existing to-day—that of Wundt—there *is* agreement upon definitions. And even if my critic's remarks were true, it would not follow that a number of wrongs made a right.

(4) I might, in my last letter, have adverted upon the term, *psycho-physiological*. I did not understand what it exactly meant. In Nature of March 29, Prof. Ll. Morgan defines it (p. 504) as the equivalent of Fechner's internal psychophysics. (*op. cit.* p. 10). In this sense it is not wanted; the phrases "external" and "internal psychophysics" are in use. (It might, however, be used to signify that part of physiology which has a conscious correlate.)

(5) My critic triumphantly adduces "reaction-times" as a subject treated of in the University College course. That course, *i.e.* deals with *one* conscious element, and with *one* type (action) of *one* of the two modes of conscious combination (association: fusion is left out of account). Prof. Münsterberg (Preface to *Psychological Laboratory of Haward University*) speaks of "the error, which is so prevalent, that experimental psychology is confined to the study of sensations and simple reaction-times."

(6) I am sorry that Dr. Hill's name should have been mentioned. I should not think of offering any opinion upon his work. I know no more of it than do the other readers of Nature. If he sees these remarks, I hope he will believe that my original criticism was meant to be quite impersonal.

(7) "By far the larger part of the really fruitful work [in psycho-physiology]" says my critic, " . . . . has been done in the investigation of the senses." If he means by psycho-physiology what Prof. Ll. Morgan does, I must disagree with him. To substantiate either view would need an article. As he writes not as a working psychophysicist (else he would have been acquainted with Fechner's *Psychophysik*), I think that the *onus probandi* lies with him.

(8) As to Prof. Ll. Morgan's paper on "the scope of psycho-physiology [= internal psycophysics]," I must plead guilty to finding the writer's eclecticism somewhat unintelligible, and his whole treatment a little general and superficial.

(9) A very interesting minor question is that of the relation of Wundt's physiology psychology to Fechner's internal psychophysics. (*cf.* Külpe, *Arch. f. Geschichte du Philosophie*, 1892, pp. 183-4.)

Cornell University, April 16. E. B. Titchener.

It seems hardly profitable to carry on a discussion with Dr. Titchener at intervals of more than a month. I readily confess that through an error of memory, for it is a good while since I read the "Elemente der Psychophysik," I misrepresented Fechner's use of the term psychophysics. The fact, however, that he recognised an "outer psychophysics," and the further fact that, as he shows ("Elemente," i. p. 11), nearly the whole of his inquiry has to do with establishing the relation of external stimuli to psychic phenomena, show that the error I fell into was not altogether unnatural. Are not the inquiries of Weber, Fechner, and their successors still brought under the head of psychophysics by those who reject Fechner's peculiar "psycho-physical" interpretation of the results? And do not nine students out of ten, who are not themselves "working psycho-physicists," associate the term "psychophysics" with these important lines of inquiry? If so, I would contend that there is room for a reconsideration of the terminology of the subject. The retention of Fechner's "outer psychophysics" seems confusing if, as I understand Dr. Titchener to say, "psychophysics' has properly to do with the correlation

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